SCIENCE

FRIDAY, FEBRUARY 25, 1916

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THE INTERRELATIONS OF PURE AND APPLIED CHEMISTRY[1]

WITHIN the past fifty years there has been a revolution in civilized industries more far-reaching in its effects than the rise or fall of dynasties or the arbitrament of war. It is a quiet, peaceful revolution, so unobtrusive that even its active agents have rarely been aware of its significance. Even the astounding efficiency of armies in the present European war is but a minor item in the forward movement.

This revolution, which is still going on, and may continue indefinitely, is both simple and complex. It is merely the gradual substitution of scientific accuracy for empiricism, of quantitative and rational methods for rule of thumb. It means better service, better wares, intelligent agriculture, improved sanitation, the suppression of epidemics, and the prevention of waste. Through its agency the luxuries of a century ago have become almost necessities; travel has been made easier and cheaper; commerce is broadened; and all the nations of the earth are now brought together in a community of interests which is only interrupted temporarily by war. Even the horrors of war are somewhat mitigated by the beneficent activities of the Red Cross service, which owes much of its effectiveness to the discoveries of science; an effectiveness which would have been impossible in the days of our grandfathers. With the aid of modern inventions the powers not at war are now able to relieve much of the suffering due to war. Steam and the telegraph have made charity more prompt and

[1] Address before Section VII. of the Pan-American Scientific Congress, January 3, 1916.

effective; while antiseptics and anesthetics, the products of chemical research, have checked the spread of disease and relieved pain.

Throughout this revolution chemistry has played and is still playing an important part. It not only touches every branch of industry, but it also reaches out into other fields of knowledge and aids their development. The geologist demands chemical data; physiology is in great part chemical; astronomy makes use of chemical discoveries whenever it analyzes the spectrum of a nebula or star.

In these preliminary remarks, I have suggested only the applications of science to the "betterment of man's estate"; but suppose there had been no science to apply. Suppose that no inquisitive mortals had ever cared to study apparently useless things, or to ponder over those obscure relations which foreshadow the discovery of natural laws. Civilization would have advanced, doubtless; but so slowly that centuries or even millenniums of progress could hardly have placed us on the level of to-day. If our predecessors had only considered mere utility, the great inventions of chemistry and electricity would never have been made. These inventions were the outgrowth of investigations that were conducted without thought of practical uses, but were searchings after truth alone.

History is full of paradoxes; and so I must seem to contradict myself when I say that the beginnings of science, the germs from which it grew, were certainly utilitarian. Discoveries were made by accident; of metals, of medicines, of dyes, and, probably earlier still, of fire. Facts, useful to mankind, were slowly collected, and in time, by the crudest of mental processes, were roughly classified. Similar things were grouped together, simple relations were observed; and these were the raw material with which science, as we understand it, began. Arts were highly developed before true science became possible. To trace their advance from savagery to civilization is one of the functions of anthropology.

The science of chemistry deals primarily with transformations of matter. Perhaps the first of these to attract attention was the change of wood to charcoal, but such transformations were doubtless taken as a matter of course, and gave rise to no serious reasoning. With the ancient Greeks, however, and perhaps earlier in Egypt, India and Crete, the accumulations of empirical knowledge led to speculations, and philosophers began to consider the absolute nature of matter. The Greek speculations are well known, but they were speculations only and bore no useful fruit. It was only after systematic experimentation had supplied a real basis for reasoning that chemical theory became possible. The Greeks were acute philosophers, but experimental work was the province of artisans, and so fact and theory rarely came together.

Slowly, however, a body of chemical doctrines developed, largely esoteric, and known only to the initiated, who had very practical aims in view. They sought to discover medicines and poisons, to transmute base metals into gold, to find a universal solvent and the elixir of life. Through their efforts many useful compounds were brought to light, but the problems they sought to solve were unsolvable. Their discoveries were the by-products of their researches, not the main object of their desires. Their speculations led to experiments, and in this union of theory, even false theory, with practise, modern chemistry began. By slow degrees empiricism developed into scientific method, and as the field of knowledge was enlarged, valid generalizations, true stimulators of rational research, were framed.

The union of theory and practise, that is the keystone of modern chemistry. Theory coordinates and arranges; practise discovers, and each one helps the other. The concrete facts of science, taken only as facts, form a disorderly and unmanageable mob; good theory converts them into a disciplined army. A thousand isolated facts are not easily remembered, theory brings them all under one general expression, and the difficulty disappears. Empirical knowledge is an aggregation of facts; theory combines them into that systematic organization which we call science. Chaos gives way to order. Theory, moreover, not mere speculation, guides research into profitable paths and makes practise more surely fruitful. The self-styled "practical man," who affects to despise theory, is apt to go astray, and to waste his time in haphazard experimenting. The Patent Office is the graveyard of many such fruitless efforts.

Let me illustrate my meaning by a concrete example: In theorizing upon the nature of matter the Greek philosophers developed an atomic speculation which was the subject of controversy, of arguments pro and con, for more than twenty centuries. It was speculation only, and it led to no definite results, for it rested upon no adequate basis of experiment.

A little more than a century ago John Dalton proposed an atomic theory which had for its purpose the correlation and explanation of certain established relations. In this respect it differed from mere speculation about what might or ought to be; it was something more than an affair of words and syllogisms, and, furthermore, it assumed quantitative form. In Dalton's hands the theory led to the discovery of those fundamental constants of matter which we call the atomic weights, with which the physical properties of the chemical elements are intimately connected. It was a fruitful theory, capable of growth, and for a hundred years it has been the chief guide of chemical research.

In the first place the atomic theory gave us, or at least made possible, our system of chemical formulæ, by which the composition of compound substances can be clearly and easily expressed. A vast number of individual data were thus brought into order, and became manageable. With these formulæ equations could be constructed and chemical arithmetic was born. Nearly all chemical calculations, especially the calculation of analyses, rest upon the constants which Dalton discovered. As a labor-saving device the atomic theory has been of enormous value. Chemical operations are also made more exact and economical by the calculations which theory has rendered possible, and wastage is avoided.

But this is not all. From the main stem of the theory subordinate theories have branched, and the theory of valency is one of them. Chemical knowledge became still more systematic and orderly, and chemists were guided into profitable lines of research. For instance, the benzene ring of Kekulé, a conception which had at first only scientific interest, led to consequences of the highest practical significance. The whole development of coal-tar chemistry, for over fifty years, with its discoveries of dyestuffs, medicines and explosives, has been systematically guided by Kekulé's generalization. Theory and practise have worked together and to mutual advantage. Pure science and applied science have both been benefited.

Between pure chemistry and applied chemistry there is no sharp line of demarcation; both are phases of one science which can not be subdivided. The difference between them is one of point of view, of purpose, or of temperament on the part of the investigator. One chemist seeks for

truth, regardless of its possible utility; another strives to apply the truth to the material welfare of mankind. The truth comes first, however; its applications only follow. The great edifice of applied science rests upon foundations of pure research. The work of Gilbert, of Galvani, of Volta, of Faraday, preceded the electrical advances of to-day. The seemingly useless discoveries of one generation have made modern inventions possible. In every department of science this principle holds true, and in none more than in chemistry. A single fact, insignificant by itself, may be the final link in an important chain of evidence.

The uses of a discovery can not be foreseen. Aniline was useless for many years after its discovery, but its importance is much in evidence to-day. Bromine and iodine were chemical curiosities at first, but they had much to do with the development of photography; an art which came into existence years after the two elements were first made known. So-called rare metals, unimportant only thirty years ago, have now found applications and are commercially valuable. Tungsten and vanadium are used in hardening steel, and tungsten also forms the filaments of incandescent lights. Thorium is utilized in the Welsbach mantle, chromium and titanium have found new uses; and the list might be indefinitely extended. Discovery came first, utilization was always much later. Modern bacteriology grew out of a controversy between two chemists, Pasteur and Liebig, who held opposing views as to the nature of fermentation. They fought over principles, and the practical consequences of the final decision could hardly have been anticipated.

Every argument has two sides. If applied chemistry owes much to pure chemistry, it has given much in return. It has stimulated research and suggested new problems. An honest investigation in the field of applied science is likely to yield some data of no immediate use in industry, but nevertheless of real scientific interest. Such data are often more than isolated facts, for they may fill gaps in our knowledge, or serve as evidence in the establishment of some principle. The search for useful derivatives of coal-tar, for example, has led to the discovery of thousands of compounds which, although commercially unavailable, have yet helped to build up the colossal structure of organic chemistry. Theory has aided practise, and practise has done much to strengthen theory. Neither side can claim absolute supremacy.

In all that I have said so far there is nothing new, at least to men of scientific training. We all know the outlines of chemical history, and can agree in a general way as to fundamental principles. But knowing and realizing are two different things. We become so accustomed to objects immediately about us that we often fail to realize their presence unless they are constantly used. It is the same with principles and ideas. The work we are actually doing absorbs our thoughts, and we forget or unconsciously ignore the equal, perhaps greater importance of other things. We know but do not realize. The most obvious truths are those which oftenest need to be recalled. They are so obvious that they no longer attract attention. On occasions like this it is permissible to emphasize them, and truisms become respectable.

I speak now to experts; but what of the layman, the employer of labor, the consumer of scientific results? How far can he be made to realize that his applications of science rest, not upon empirical experimentation, but upon a long line of seemingly abstract researches, guided by theories which to him appear to be visionary?

To this question no general answer can

be given, and for obvious reasons. Some manufacturers are ignorant and stupid, the ultra-conservatives; others are intelligent, progressive, wide-awake. Great advances, however, have been made, and the good work still continues. The older men among us can remember the time when American mills and factories rarely employed a chemist, except when difficulties were encountered which could only be solved by analysis. Even then the cost of the work was paid most grudgingly as if it were an extravagance which should have been avoided. Now it is usual for manufacturing corporations to maintain laboratories, in which chemists, too often underpaid, are regularly employed. Some companies, the General Electric Company, for example, spend large sums of money on research, but others are more niggardly. Here we have much to learn from Germany. Her great advances in chemical industries have been made possible by the employment of trained investigators, whose duty it is to discover new products of value and to improve processes. Men who had shown ability in the solution of unsolved problems were chosen for this work, and not mere analysts only. In Germany, more than in any other country, has the commercial value of scientific intelligence been realized. The routine man has his place, but the thinker outranks him. When American employers are willing to spend as much time and money on research as they now spend on law, their economic conditions will be much improved. The chemist who solves an important problem, or who shows how to avoid waste, might well be paid as much as the lawyer, who, after all, may only lose his case. Although we are improving, we still have far to go.

A congress of this kind is of slight importance unless it can bring forth suggestions which shall help in the future advancement of science. It is, of course, pleasant to meet together, to compare notes and to form new friendships, but something more serious and permanent is demanded. What does science need, and what are its weak points? These are questions worth considering.

So far, with few exceptions, science has advanced through the efforts of individuals, and not by any definite system. The result is, especially in chemistry, an ill-balanced body of knowledge, overdeveloped in some directions, underdeveloped in others. The individual studies the subject which interests him and has attracted his attention, and too often fails to think of chemistry as a whole. Our knowledge is full of gaps, and these frequently occur where one would least expect to find them. We know many physical constants, for example, but for no single substance have all the desirable data been determined. This is a condition which should be remedied—but how?

The essential thing, it seems to me, is that there should be greater cooperation among investigators, and a subordination of personal interests to the general welfare. There are individual geniuses, of course, whose imagination reaches out into the unknown, and brings back wonderful discoveries; but such men must work alone and never in harness. They are the glorious few; I speak for the laborious many. Nor do I suggest any check to individual enterprise, only that it should be supplemented and helped by some intelligent system.

In every department of science there are problems too large for any single worker to handle, and here cooperation is possible. In this direction astronomers have set us an example, and observatories now combine their resources in mapping the starry heavens. Each observatory takes a definite zone, and the work goes on systematically. Such cooperation is practicable, and it leads to

permanent results. A definite field of work is definitely divided, and then cultivated under a preconcerted plan.

In chemistry, however, institutions equivalent to astronomical observatories can hardly be said to exist. Therefore, it is desirable that they should be created. Laboratories for systematic research are needed, in which bodies of trained men can work together for the common welfare. The work most needed to be done is not showy, but laborious; it will bring little fame to the individual, whose personal interests, however, need not be wholly disregarded.

To make my meaning clear I may cite one line of investigation which might be taken up, the importance of which I have discussed on several previous occasions. The great, fundamental problem which I have in mind is this: what relations connect the physical properties of compounds with those of their component elements? How can we calculate the one from the other?

The first thing to do, evidently, is to determine with accuracy the physical constants of the elements themselves; for just here our present knowledge is wretchedly incomplete. Take iron, or gold, or copper, for instance; how much do we know of their fundamental properties? A fraction only, a small fraction of what should be known. Here, then, is one line of work for an organized laboratory to do; one which would lay the foundations for great generalizations. Each constant should be measured throughout the entire range of attainable temperature; excepting only those which hold for one temperature alone. To accomplish all this new methods would have to be devised, and new instruments invented; and this would be of service to industrial enterprises as well as to science. The great revolution of which I spoke at first would be still farther advanced, precision would replace present uncertainty; all chemistry and all physics, the Siamese twins of science, would reap unforeseeable advantages.

A modern dreadnought costs, with its equipment, fifteen millions of dollars. It may be sunk by a torpedo in the first week of its career, or it may last twenty-five years, never meeting an enemy, and then be discarded as obsolete. The battleship is necessary, no doubt, at least as society is now organized; but it is unproductive, an instrument of destruction, and, therefore, perhaps unavoidably, a waste.

Fifteen millions of dollars! For one fifth of that sum a laboratory for research could be built, equipped and permanently endowed, which would benefit mankind for centuries to come. Surely some of the wealth which chemistry has created might well be devoted to such an enterprise as I am advocating now. Libraries, observatories and museums have all been enriched by private beneficence, but here is something of no less merit for which no provision has been made. Let us hope that the forward step may first be taken somewhere within the Western Hemisphere.

Between pure and applied science, or, rather, between the scientific investigator and the so-called "practical" man, there is often, but not always, an unfortunate difference. The worker in pure science publishes his discoveries to the world, regardless of commercial values. The manufacturer, on the other hand, who pays or thinks he pays for scientific investigations, is apt to keep his results secret, in order that he may turn them to personal profit. This policy of secrecy, too often followed, is bad for science and for industry. Science is deprived of useful data, which might add greatly to its advancement. Manufacturers waste their time and money in duplications of research, or, frequently, in rediscovering that which is already well

known. I have myself seen a supposedly "secret" process which had been in print for many years and was doubtless known to all competitors. Temporary secrecy, pending applications for patents, is of course not objectionable, but permanent secrecy is wrong. The man who uses science in developing his industry owes something to science in return. In the long run, moreover, publicity regarding scientific investigations is profitable. With a liberal policy, each manufacturer would give out his own small contributions to science, and receive the results obtained by all others in return. The practise of secrecy, to use the common phrase, is penny wise and pound foolish.

I plead, therefore, not only for cooperation in pure research, but also for greater cooperation, for more reciprocity between investigation and industry. The application of science to human welfare is glorious; its selfish uses are at least not praiseworthy. The devotee of pure science and the technologist should seek to understand each other, and to realize that the conduct of research involves mutual responsibilities. We may not attain to our ideals, but we can surely move towards them.

To-day the thoughts of the civilized world are turned towards war, and all men are longing for the peace which must come, sooner or later. As one of our earliest poets has said:

> War ends in peace, and morning light
> Mounts upon midnight's wing.

That is true of material warfare, but we are engaged in a conflict which, fortunately, can never end. It is the war of intelligence against the inertia of ignorance, and it keeps intelligence alive. Ignorance will always exist; the unknown will always be vaster than our knowledge, but we may hope for many future victories, and fear no ruinous defeats. So long as science lives it must move forward, driven by a splendid discontent with our deficiencies. May we never be satisfied, and forever advance, safe in the conviction that every conquest of ours over ignorance means the greater welfare of mankind.

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THEODOR BOVERI[1]

WITHIN a single year after Weismann's death our science has suffered another severe blow in the loss of Theodor Boveri, who died in Wuerzburg on October 15 at the age of fifty-three years. Pioneer and leader in the fields of cytology and experimental zoology, his loss will be felt keenly in this country where he had so many friends and pupils and where his field of research has been so popular during the past two decades. Boveri's personal life was very simple, always devoted to his work, his family and the pleasure coming from a deep love for art and nature. A native of Bavaria, he studied first philosophy and later zoology in Munich. His doctor's thesis on the structure of the nerve fibers in vertebrates treated a subject to which he did not later return. For, encouraged by his teacher, Richard Hertwig, soon after receiving his degree he entered the field of cytological research. Here, following the example of his teacher, he combined practically from the beginning the morphological and experimental methods.

His very first work in this line proved to be a great success, securing to him the *venia legendi* as privat dozent in the University of Munich. A few years later, when only thirty years of age, he was called to Wuerzburg, to succeed Semper in the chair of zoology and comparative anatomy. Here he remained during the rest of his life with the exception of frequent trips to the zoological stations of southern Europe, especially Naples, where he was a regular guest. He also made a short visit to the United States. His reputation as

[1] Paper read before the Biological Club, Yale University, December 3, 1915. I am greatly indebted to Professor Wesley R. Coe for kindly revising the manuscript.